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BEHAVIOR ANALYSIS: THRIVING, BUT HOW ABOUT ITS FUTURE?

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Behavior analysis has been thriving by continuing to make important theoretical and empirical contributions to a wide array of problems, as well as by contributing to interdisciplinary research. Applied research in behavior analysis is flourishing. Despite these positive signs there may be an erosion of support for basic research in animal learning and behavior, including behavior analysis. Increased attention by behavior analysts to fundamental problems in areas of cognition, including decision-making and language, may help behavior analysis to evolve more successfully. An experimental analysis of gambling may prove particularly fruitful.

Key words: behavior analysis, judgment, decision-making, gambling

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I served as Editor for the issues published in 1988 through 1991. Although the Journal was in superb shape when I took over the Editorship from Phil Hineline, as with any editor I had a few ideas for shaping the emphasis of the Journal; and, I hoped, indirectly, to have an impact on behavior analysis. That I believe my actions had precious little impact does not deter me from looking back at those aims and addressing the goals I would have were I embarking on my editorial assignment twenty years and five editors later. Nor will I be deterred by the arguably embarrassing fact that my goals today would be the same ones I had twenty years ago. Although there has been great progress in behavior analysis in the past twenty years, I believe that our impact continues to be limited to a degree by our insularity and by an inflated perception of that insularity among those outside the behavior-analytic tradition with whom we should be collaborating. In my introductory Editorial I noted:

> In the past, many behavior analysts have shied away from the study of cognitive phenomena. True, there are several exceptions—for example, the writings of B.F. Skinner; studies of concept formation, especially natural concepts; behavioral approaches to timing; and many empirical investigations of memory with delayed-matching-to-sample techniques. But behavior analysts have certainly not been at the forefront of the recent movement in cognition. This is disappointing because behavior analysts, more than any other group, should have important statements to make and fruitful experiments to conduct in a functional analysis of language and cognition…I believe that basic research in behavior analysis has much to gain by contemplating our research issues in the context of related perspectives such as the biological and the cognitive. Phenomena addressed in these areas have relevance for our own work, and we are often in an optimal position to make important contributions to the analysis of these phenomena. Behavior analysts are sometimes as guilty as other psychologists of pursuing their particular basic research interests narrowly without pausing sufficiently to appreciate the potential ramifications of that research. Yet of all psychologists, behaviorists have the most to offer to contemporary psychology in its broadest sense. In addition, too seldom do authors in this journal make clear the applied implications of their research. Contemplation of the applied relevance of basic research may lead to important ideas for basic work (just as consideration of the empirical underpinnings of applied research may lead to new ideas for applied work). (Fantino, 1988, p. 1).

I still believe we should be doing more in the way of addressing traditional problems of interest to psychologists in other areas, including cognitive, developmental, social, and physiological psychology. I believe we have been rewarded when we have taken a more interdisciplinary approach to the study of behavior. I am absolutely *not* recommending that we eschew a behavior-analytic approach. I am simply suggesting that we use that approach more frequently to address traditional issues in psychology and that we do so with a degree of clarity that would ensure that our message is well understood by other psychol-

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ogists who are at least somewhat receptive to alternative approaches, as long as those approaches can be demonstrated to be fruitful and relevant. The importance of ''relevance'' has been underscored recently by a greater insistence at NIMH that grant proposals address the mission of the agency. In this brief essay, I will mention a few cases where our eclecticism has been effective in the past and discuss one area that may be the focus of productive interactions in the immediate future. First, a quick review of some actions taken by the Journal during my term and some developments since.

To encourage an increase in breadth we published a special issue of the Journal in each of the four years of my term as Editor. These focused on Behavior Analysis and Biological Factors (1988), Cognition and Behavior Analysis (1989), The Experimental Analysis of Human Behavior (1990) and Behavioral Pharmacology (1991). The Journal has continued to attract major articles in behavioral pharmacology (20% of experimental articles in 2004, 23% in 2005, 8% in 2006). An emphasis on the experimental analysis of human behavior also has been maintained with a sizeable portion of articles studying human subjects (24% of experimental articles in 2004, 23% in 2005, 29% in 2006). The thickness (not to mention the high quality) of the recent Special Issue on The Relation of Behavior and Neuroscience (2005), with 21 distinguished contributions, reflects continued success in attracting the interest of cutting-edge researchers at the intersection of behavior analysis and neurosciences. Thus, it would be easy to conclude that we are moving in the right direction in terms of interdisciplinary breadth and relevance to the mainstream of experimental psychology. This essay deals briefly with what I see as the major accomplishments and limitations of this quest for relevance as well as with the issue of whether or not such relevance (or ''mainstreaming'') is desirable.

It is clear from a glance at the list of Editorial Board members that the Journal emphasizes the breadth of behavior analysis. One of the most constructive developments in our field has been the popularity of the Society for the Quantitative Analysis of Behavior (SQAB). These meetings have attracted a wide following that includes researchers from several disciplines including behavioral ecologists, behavioral economists, associative learning theorists, and others interested in the quantitative analysis of behavior, as well as, of course, behavior analysts. Organizers of the meetings have encouraged this catholicism by inviting distinguished speakers from related disciplines. In recent years these have included Gerd Gigerenzer, Paul Glimcher, Steve Grossberg, Michael Kubovy, David Laibson, W. Todd Maddox, Michael Platt, Barbara Mellers, and Richard Selten. The continued growth of the Association for Behavior Analysis (ABA) as well as its local affiliates also speaks to the fact that behavior analysis is thriving. But if the journals and organizations of behavior analysis have reached out to researchers in related disciplines, have these overtures been reciprocated? In several respects the answer is ''Yes''. For example, several behavior analysts have been invited to give major addresses at nonbehavioral meetings. A *JEAB* article on memory won the APA's George Miller award for best experimental article of the year (White & Wixted, 1999). Importantly, a key granting agency panel (Biobehavioral Regulation, Learning and Ethology, BRLE, of NIMH) is well represented by behavior analysts and by others sympathetic to and knowledgeable about behavior analysis (indeed, Marc Branch, a former *JEAB* Editor is serving as the panel's chairperson).

For all these positive developments, however, there are still worrisome clouds on the behavioral horizon. Applied behavior analysis is flowering, but despite advances made in basic behavior-analytic research, I have the strong impression that behavior analysis is not doing quite as well in academia. In part this reflects budgetary and programmatic constraints that have caused retrenchment of animal behavior courses (particularly lab courses) and animal research laboratories. To some degree this reflects a shift of emphasis from animal learning (including behavior analysis) to neuroscience. But key behavioral vacancies at several major universities have been filled with candidates from other disciplines.[1] There may be no immediate conse-

[1] On a personal note, rather than retire I feel compelled to retain my position at UCSD in order to keep the faculty line in behavior analysis and to ensure that the undergraduate laboratory in operant psychology (a pigeon lab) that I have taught for 40 years can continue to be offered.

quence of this shift. However, I worry that a reduced presence in academia may result in fewer basic researchers. One side effect of such a development would be a drying up of influence on applied research. It is possible that by making our research more accessible to non–behavior analysts we may improve our likelihood of securing academic research and teaching positions.

What successes have we had since the 1980s? In that period we were experiencing a rich interaction with behavioral ecologists that resulted in conferences and book volumes bringing behavior analysts and behavioral ecologists together. Subsequently there was a great deal of interest in both empirical studies and in quantitative theories of timing. There also has been some interdisciplinary research in the areas of memory and in judgment and decision-making that has drawn the attention of cognitive scientists (as well as cognitive psychologists) to the promise of behavior-analytic research (e.g., Goodie & Fantino, 1996; White & Wixted, 1999; Wixted, 1989). Of enduring significance, the area of behavioral economics, jointly developed by behavioral economists and behavior analysts, has brought together scholars from these disciplines as well as from behavioral ecology and cognitive science. Are there other areas that are likely to fuel further interdisciplinary advances?

My candidate is research on gambling. Gambling is a general topic that offers opportunities for basic research and theory, and one that has hugely important applied implications. The current view of pathological gambling as an addiction cries out for a functional analysis of the controlling variables and for strategies of behavioral intervention. Interest in gambling cuts across many areas of psychology. Behavior analysts have stressed the relevance of principles of self-control and temporal discounting (pathological gamblers have steeper discounting functions) and are looking for the various reinforcers that maintain gambling. For example, what is the significance of emotional and cognitive accompaniments of placing wagers, anticipating outcomes, and receiving (or not receiving) payoffs? I submit that the effects of these variables may be best understood within a behavioral framework. I believe that this is an area that will see important and well-publicized advances in the next few years and that behavior analysts may be in the forefront of these advances. Gambling research offers yet another important arena in which biological, cognitive, and behavioral perspectives intersect. I look forward to seeing my colleagues' articles on this and other topics of broad interest in *JEAB* in the years to come.

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